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parts at once, and with the eyes directed to the chest, the relation of the movements and of the sounds, normal or abnormal, of this most important organ is very fully and satisfactorily made out.

**POSTSCRIPT.** Received April 22, 1858.

In connexion with that part of my paper which treats of the restriction of hearing to the closed ear, I desire to add the fact which I have ascertained within the last few days, that if one ear be closed wholly or partially at its external part, *i. e.* at the meatus externus, by disease or by congenital malformation, while the other ear is healthy, the sound of the tuning-fork, applied to any part of the head, is heard only in the closed ear. This fact holds, although the closed ear is totally unaffected by sounds conveyed through the external air.

I have further to mention the fact, that all persons, deaf in one ear, whom I have lately examined, with one exception, hear the sound of the tuning-fork applied to the head in that ear only that is deaf to external sounds. A man who has been totally deaf in one ear for thirty years, in consequence of a violent blow upon the head, had the tuning-fork applied over the forehead. He started, and said that he heard only in the ear which had been deaf during that long course of time. In such cases I have been disposed to believe that, amidst other lesions of the organ of hearing, there may be present an obstruction or closure, that a reverberation takes place, and that thus a restriction of hearing is secured for the diseased organ.

**II. "On the Stratification of Vesicular Ice by Pressure."** By Professor WILLIAM THOMSON, F.R.S. In a Letter to Professor STOKES, Sec. R.S. Received April 3, 1858.

In my last letter to you I pointed out that my brother's theory of the effect of pressure in lowering the freezing-point of water, affords a perfect explanation of various remarkable phenomena involving the internal melting of ice, described by Professor Tyndall in the Number of the 'Proceedings' which has just been published. I wish now to show that the stratification of vesicular ice by pressure observed on a large scale in glaciers, and the lamination of clear ice described by Dr. Tyndall as produced in hand specimens by a

Brahmah's press, are also demonstrable as conclusions from the same theory.

Conceive a continuous mass of ice, with vesicles containing either air or water distributed through it; and let this mass be pressed together by opposing forces on two opposite sides of it. The vesicles will gradually become arranged in strata perpendicular to the lines of pressure, *because of the melting of ice in the localities of greatest pressure and the regulation of the water in the localities of least pressure, in the neighbourhood of groups of these cavities.* For, any two vesicles nearly in the direction of the condensation will afford to the ice between them a relief from pressure, and will occasion an aggravated pressure in the ice round each of them in the places farthest out from the line joining their centres; while the pressure in the ice on the far sides of the two vesicles will be somewhat diminished from what it would be were their cavities filled up with the solid, although not nearly as much diminished as it is in the ice between the two. Hence, as demonstrated by my brother's theory and my own experiment, the melting temperature of the ice round each vesicle will be highest on its side nearest to the other vesicle, and lowest in the localities on the whole farthest from the line joining the centres. Therefore, ice will melt from these last-mentioned localities, and, if each vesicle have water in it, the partition between the two will thicken by freezing on each side of it. Any two vesicles, on the other hand, which are nearly in a line perpendicular to the direction of pressure will agree in leaving an aggravated pressure to be borne by the solid between them, and will each direct away some of the pressure from the portions of the solid next itself on the two sides farthest from the plane through the centres, perpendicular to the line of pressure. This will give rise to an increase of pressure on the whole in the solid all round the two cavities, and nearly in the plane perpendicular to the pressure, although nowhere else so much as in the part between them. Hence these two vesicles will gradually extend towards one another by the melting of the intervening ice, and each will become flattened in towards the plane through the centres perpendicular to the direction of pressure, by the freezing of water on the parts of the bounding surface farthest from this plane. It may be similarly shown that two vesicles in a line oblique to that of condensation will give rise to such

variations of pressure in the solid in their neighbourhood, as to make them, by melting and freezing, to extend, each obliquely *towards* the other and *from* the parts of its boundary most remote from a plane midway between them, perpendicular to the direction of pressure.

The general tendency clearly is for the vesicles to become flattened and arranged in layers, in planes perpendicular to the direction of the pressure from without.

It is clear that the same general tendency must be experienced even when there are bubbles of air in the vesicles, although no doubt the resultant effect would be to some extent influenced by the running down of water to the lowest part of each cavity.

I believe it will be found that these principles afford a satisfactory physical explanation of the origin of that beautiful veined structure which Professor Forbes has shown to be an essential organic property of glaciers. Thus the first effect of pressure not equal in all directions, on a mass of snow, ought to be, according to the theory, to convert it into a stratified mass of layers of alternately clear and vesicular ice, perpendicular to the direction of maximum pressure. In his remarks "On the Conversion of the Névé into Ice\*", Professor Forbes says, "*that the conversion into ice is simultaneous*" (and in a particular case referred to "*identical*") "*with the formation of the blue bands*; . . . and that these bands are formed where the pressure is most intense, and where the differential motion of the parts is a maximum, that is, near the walls of a glacier." He farther states, that, after long doubt, he feels satisfied that the conversion of snow into ice is due to the effects of pressure on the loose and porous structure of the former; and he formally abandons the notion that the blue veins are due to the freezing of infiltrated water, or to any other cause than the kneading action of pressure. All the observations he describes seem to be in most complete accordance with the theory indicated above. Thus, in the thirteenth letter, he says, "*the blue veins are formed where the pressure is most intense and the differential motion of the parts a maximum.*"

Now the theory not only requires pressure, but requires difference of pressure in different directions to explain the stratification of the vesicles. Difference of pressure in different directions produces the "*differential motion*" referred to by Professor Forbes. Further,

\* Thirteenth Letter on Glaciers, section (2), dated Dec. 1846.

the difference of pressure in different directions must be continued until a very considerable amount of this differential motion, or distortion, has taken place, to produce any sensible degree of stratification in the vesicles. The absolute amount of distortion experienced by any portion of the viscous mass is therefore an index of the persistence of the differential pressure, by the continued action of which the blue veins are induced. Hence also we see why blue veins are not formed in any mass, ever so deep, of snow *resting* in a hollow or corner.

As to the direction in which the blue veins appear to lie, they must, according to the theory, be something intermediate between the surfaces perpendicular to the greatest pressure, and the surfaces of sliding; since they will commence being formed exactly perpendicular to the direction of greatest pressure, and will, by the differential motion accompanying their formation, become gradually laid out more and more nearly parallel to the sides of the channel through which the glacier is forced. This circumstance, along with the comparatively weak mechanical condition of the white strata (vesicular layers between the blue strata), must, I think, make these white strata become ultimately, in reality, the surfaces of "sliding" or of "tearing," or of chief differential motion, as according to Professor Forbes's observations they seem to be. His first statement on the subject, made as early as 1842, that "the blue veins seem to be perpendicular to the lines of maximum pressure," is, however, more in accordance with their mechanical origin, according to the theory I now suggest, than the supposition that they are *caused* by the tearing action which is found to take place along them when formed. It appears to me, therefore, that Dr. Tyndall's conclusion, that the vesicular stratification is produced by pressure in surfaces perpendicular to the directions of maximum pressure, is correct as regards the mechanical origin of the veined structure; while there seems every reason, both from observation and from mechanical theory, to accept the view given by Professor Forbes of their function in glacial motion.

The mechanical theory I have indicated as the explanation of the veined structure of glacial ice is especially applicable to account for the stratification of the vesicles observed in ice originally clear, and subjected to differential pressure, by Dr. Tyndall; the formation of the vesicles themselves being, as remarked in my last letter\*, anticipated

\* See Proceedings for February 25, 1858.

by my brother's theory, published in the 'Proceedings' for May 1857.

I believe the theory I have given above contains the true explanation of one remarkable fact observed by Dr. Tyndall in connexion with the beautiful set of phenomena which he discovered to be produced by radiant heat, concentrated on an internal portion of a mass of clear ice by a lens; the fact, namely, that the planes in which the vesicles extend are generally parallel to the sides when the mass of ice operated on is a flat slab; for the solid will yield to the "negative" internal pressure due to the contractility of the melting ice, most easily in the direction perpendicular to the sides. The so-called negative pressure is therefore least, or which is the same thing, the positive pressure is greatest in this direction. Hence the vesicles of melted ice, or of vapour caused by the contraction of melted ice, must, as I have shown, tend to place themselves parallel to the sides of the slab.

The divisions of the vesicular layers into leaves like six-petaled flowers is a phenomenon which does not seem to me as yet so easily explained; but I cannot see that any of the phenomena described by Dr. Tyndall can be considered as having been proved to be due to ice having mechanical properties of a uniaxial crystal.

*April 29, 1858.*

J. P. GASSIOT, Esq., Vice-President, in the Chair.

The following communications were read:—

I. "An Account of the Weather in various localities during the 15th of March, 1858 (the day of the Great Solar Eclipse); together with Observations of the Effect produced by the Diminution of Light upon the Animal and Vegetable Kingdoms." By EDWARD JOSEPH LOWE, Esq., F.R.A.S., F.G.S., F.L.S., F.Z.S. &c. Communicated by THOMAS BELL, Esq., P.L.S. Received April 1, 1858.

[Abstract.]

By the author's request, observations were made at 9 A.M., 11 A.M., and from noon every fifteen minutes up to 2<sup>h</sup> 16<sup>m</sup>, at 3 P.M.,